JEL Classification: H31, H51, I18
Keywords: doctor visit co-payments, children, difference-in-difference

Doctor-Visit Co-Payment Exemption for Children: First Look at the Data*

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Abstract
This paper uses the abolition of children's doctor-visit co-payments effective since April 2009 as a natural experiment to estimate the effect of those co-payments on the number of doctor visits made by children. As the policy change involved only children, we are able to use the adult part of the population as a control group in a difference-in-difference estimation. The paper approximates the number of doctor visits by consumption of prescription drugs, as visiting a doctor is the only way to obtain a prescription. Using three different pre-reform periods (January, February, and March 2009) and two post-reform periods (April and May 2009) the estimates reveal no overall effect of doctor-visit co-payments on the number of children's doctor visits. Less convincingly and more tentatively, the estimates suggest a strategic shift of children's doctor visits away from the last pre-reform toward the first post-reform month.

1. Introduction

The introduction of regulatory co-payments in Czech health care in January 2008 was one of the biggest policy changes in this sector in recent decades. The policy change received wide media attention and initiated a fierce public and political debate. Regarding the impact of the reform, the official position of the Ministry of Health is that co-payments fulfill their role in limiting the overuse of medical services and prescription drugs (see Ministry of Health of the CR, 2008b,c, 2009, press releases).

One problem with the evidence presented by the ministry is that it is based on a time-series-like comparison of data from the pre- and post-reform periods. As a result, the presented changes may have been driven by other factors and erroneously ascribed as reform effects. Claiming that the extensive changes in health service utilization observed during 2008 were driven by factors unrelated to the reform would probably be taking this argument too far. On the other hand, it is unlikely that all the changes were driven by the reform.

Another perspective we lack in the discussion is the effect of the reform on different parts of the population. The main rationale for the co-payments was to limit overuse in the health care sector. However, the extent of overuse is likely to be minimal for certain groups of people and any decrease in health service utilization is likely to be detrimental to their health. Despite the fact that there is little evidence to support this claim, we think that children are a prime example of such a group.

* Financial support from Czech Science Foundation grant 402/08/0501 "Political Economy of Public Spending" is greatly appreciated.

Those two observations motivate our research. We investigate the effect of the doctor-visit co-payment exemption for children introduced in April 2009 on children's doctor visits. As the change did not affect the adult population we are able to use adults as a natural control group against which we measure changes in the utilization of health services by children. That is, using difference-in-difference (DD) methodology we are able to filter out general time trends potentially confounding results based on time-series estimation methods.

As data about the number of children and adult doctor visits are not readily available, we proxy doctor visits by prescription drug consumption. As a doctor visit is a precondition for obtaining a prescription, we estimate the impact of the children's doctor-visit co-payment exemption on children's drug consumption, which allows us to draw conclusions regarding children's doctor visits.

To preview our results, we find no evidence of an impact of doctor-visit co--payments on children's doctor visits. This suggests that the original introduction of the co-payments had no detrimental effect on children's health outcomes. On the other hand, we find evidence of a transitory strategic timing effect. The amendment to the law regulating co-payments was signed by the president on February 18, 2009. This made it certain that the change would indeed take place. What our data show then is a statistically significant decrease in children's doctor visits in the period immediately preceding the policy change.

The rest of the paper is organized as follows. We survey the existing literature on the effects of co-payments on demand for health care services in the next section. Section 3 describes our data and methodology. We present the estimation results and robustness checks in sections 4 and 5, respectively, while section 6 concludes.

Before proceeding, we would like to comment on the terminology used in this paper. We use the terms co-payments, fees, and charges to denote the regulatory co--payments introduced in the Czech Republic in 2008 for doctor visits, prescription drugs, and some other services at a fixed amount CZK 30 (approximately €1). We do not have in mind other payments patients make, such as those for certain administrative doctor services or for covering the difference between the price of a drug and the amount covered by the public system.

2. Literature Survey

The literature investigating the effects of co-payments on the use of medical services is not very voluminous. We survey here studies related to our work either geographically (the European, not the US, experience) or topically (studies dealing with children or prescription drugs) and refer the reader to Zweifel and Manning (2000) for an extensive survey of the literature.

Two studies investigate the German experience with co-payments. First, Winkelmann (2004) estimates the effect of the 1997 increase in prescription drug co-payments on the number of doctor visits. The estimates show that the number of doctor visits fell by 10% as a result of a 200% increase in prescription drug co-payments. Another study – by Augurzky, Bauer, and Schaffner (2006) – investigates the effect of the introduction of a €10 doctor-visit co-payment in 2004 on the number of doctor visits. However, the study finds no significant effect.

Both German studies employ DD methodology using children, exempt from co-payments in both reforms, as a control group. Consequently, their treatment group is the adult part of the population. Moreover, Winkelmann (2004) uses people with private insurance and low-income people, exempt from the co-payments, as additional controls.

Another set of studies comes from the UK and investigates the impact of prescription drug fees introduced in 1968 on drug utilization. In this respect, O'Brien (1989) investigates data from the 1969–1986 period and Hughes and McGuire (1995) from the 1969–1992 period. Both studies use time-series regression due to a lack of available controls and estimate the elasticity of demand for drugs to be -0.33 and -0.37, respectively.

Besides geographically related results, there is a series of studies using data from the RAND Health Insurance Experiment (HIE) focusing on children and prescription drugs. The HIE, conducted in the mid-1970s in the US, was a controlled randomized experiment designed to investigate the effect of cost-sharing on demand for health care services. Several thousand subjects were assigned to groups with different co-insurance rates (0, 25, 50, 95) and data were collected on their utilization of health care services and health outcomes (see Manning, Newhouse, Duan, Keeler, and Leibowitz, 1987, and Gruber, 2006, for an overview of methodology and results).

Using the HIE data, Leibowitz, Manning, Keeler, Duan, Lohr, and Newhouse (1985) investigate the effect of health insurance generosity on the demand for health care by children. The results show that demand for outpatient care is responsive to health insurance generosity, with children in more generous plans consuming more outpatient services. On the other hand, no effect on inpatient care demand was found.

Leibowitz, Manning, and Newhouse (1985) present the results of the same experiment for prescription drugs, with results similar to those for outpatient care, i.e., higher demand for drugs by subjects in more generous health insurance plans.

Finally, Manning, Bailit, Benjamin, and Newhouse (1986) present the HIE results for dental care. What is interesting about this study is that it explicitly tracks the effects of the experiment in its different years. The results suggest the presence of a strategic timing effect in demand for dental care. In the first year of the experiment, the demand for dental care was significantly higher than in the second year. But more importantly, there was a significant increase in the demand for dental care in the last year of the experiment.

Overall, none of the studies in the HIE explicitly uses DD methodology. Instead, they compare the means of the collected data. On the other hand, this amounts to DD estimation, as the HIE was a randomized experiment, so that there are no ex--ante systematic differences between groups with different health insurance plans.

None of the studies mentioned above uses data about drug consumption to proxy for the number of doctor visits as this paper does. The German studies use a German socio-economic panel which tracks the number of doctor visits. The UK studies investigate drug consumption in first place, and the HIE, by design, produced very detailed data about health care utilization.

Finally, from a theoretical perspective, the relationship between the generosity of health care insurance (via lower co-payments, deductibles, co-insurance) and

health care utilization is a positive one (for a formal treatment, see Zweifel and Manning, 2000, section 5.1). With the claim that more generous health care insurance increases health care utilization not controversial, the empirical literature just discussed essentially tries to estimate the strength of this relationship.

3. Reform, Data, and Methodology

In this section we describe the reform under investigation in more detail, describe the construction of our data used in the estimation below, and explain the methodology used.

Originally, public health care sector co-payments were introduced in January 2008 as a measure to limit wasteful utilization of health care. Only minor groups were exempt, namely, people receiving low-income support benefits, children in foster homes, people in court-ordered treatment, and people with infectious diseases. The co-payments were set to CZK 30 (€1) per doctor visit and prescription item, CZK 60 (€2) per day in hospital care, and CZK 90 (€3) per visit to the emergency services. An annual limit on co-payments of CZK 5,000 (€170) was introduced at the same time (see Ministry of Health of the CR, 2008a, for more details). A few minor changes were made in August 2008.

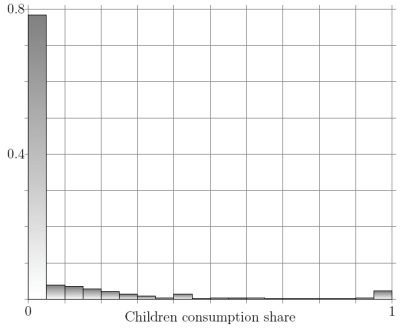
The reform we investigate is the change to the co-payment scheme described above effective since April 2009. The first change revoked doctor-visit co-payments for children. The second change decreased the annual limit for co-payments to CZK 2,500 (€85) for children and people over 65. Finally, the third change limited prescription co-payments to instances where the patient supplementary payment is less than CZK 30, with the supplementary payment being the difference between the price of a drug and the amount covered by the public system.

It is the effect of the first change that we investigate. Regarding the second change, we are unable to estimate its effect, which in itself is likely to be limited to a small fraction of society. For example, in 2008 during the first six months of the new system only 1,306 people reached the co-payment limit (Ministry of Health of the CR, 2008b), which constitutes approximately 0.01% of the Czech population. Regarding the third change, we are unable to estimate its impact either, as it affected the whole population across the board. On the other hand, as the discussion of the methodology below will make clear, the third change cannot possibly confound our results.

To fully understand the impact of the reform it is instructive to know the timing of its codification. The original, more generous version was approved by the Chamber of Deputies (lower house) on December 19, 2008 and sent to the Senate (upper house). The Senate approved a less generous version and sent it back to the Chamber of Deputies on January 28, 2009. The less generous version was then passed by the Chamber of Deputies on February 11, 2009, and was signed by the president on February 18, 2009. By then it had become clear that the changes envisioned in the less generous version would indeed become reality. On the other hand, given the political salience of the issue, the nature of the changes was hard to predict until the final signing.

To investigate the effect of the co-payment exemption for children on their doctor visits, we use data on prescription drug consumption. Under the assumption that a doctor visit generates a prescription with a fixed probability before and after

Figure 1 Percentage of Drugs with Given Child Consumption Shares



the reform, data on prescription drug consumption are a good proxy for the number of doctor visits. Notice also that we need this probability to stay fixed over the period we study, but we can allow it to vary between children and adults provided we adjust the data accordingly.

The original data we use to construct our dataset contains information from over 14,000 prescriptions taken out in a single Prague pharmacy in the period January 5, 2009 to May 31, 2009 (we call this the "observed" period). We dropped all observations from people who reached their 18th birthday in the observed period, as it is not clear whether they should count as children or adults in our study.

The use of data from a single pharmacy immediately raises two issues with our methodology. The first issue is the external validity of our results. While we are aware of the problem, there is nothing we can do about it, as a larger sample of data is simply not available. At the same time, we are not aware of any characteristic of the pharmacy (location, being part of distribution channel, being a chain pharmacy, opening hours) which would make it in any sense special.

The second issue is the fact that several regional governments were paying prescription co-payments in selected pharmacies on behalf of their citizens during the observed period. This, however, should not compromise our results, as it took place mainly in Central Bohemia and several other parts of the Czech Republic, not in Prague.

For each drug in the data we calculated its consumption over the whole observed period by children on the one hand and by adults on the other. Subsequently, we label all drugs with a child consumption share of above 0.217 as children's drugs and all drugs with an adult consumption share of above 0.783 as adult drugs. The former constitutes our treatment group and the latter our control group.

The rationale behind the cut-off is the following. The ratio of children to the adult population in the Czech Republic is 1:4.6 (Czech Statistical Office, 2009). Hence, drugs prescribed equally to children and adults will have a child consumption share equal to $\frac{1}{4.6} \doteq 0.217$ and an adult consumption share equal to $1 - \frac{1}{4.6} \doteq 0.783$. *Figure 1* shows a histogram of the numbers of drugs with given child consumption shares.

What *Figure 1* shows is that most of the drugs in our sample are prescribed predominantly to adults. We also experimented with classifying child and adult drugs

Table 1 Number of Prescriptions

	January	February	March	April	May	total
children	331	342	301	377	333	1684
adults	2590	2480	2551	2789	2676	13086

using data from the pre- and post-reform periods only. Overall, approximately 5% of the drugs display a non-stable classification into child and adult ones, with rarely prescribed drugs with low consumption being the most problematic. For this reason, in some specifications below we use a different cut-off for the classification. This more demanding classification labels as child (adult) drugs those with a ratio of child (adult) consumption of above 0.95. With this cut-off, none of the drugs switches from being an adult drug to being a child drug or *vice versa*, based on the pre- and post-reform period data.

Next, we drop all observations with adult drugs going to children and *vice versa* so that the resulting data include only adult drugs consumed by adults and child drugs consumed by children. To proceed, we split the data into five periods: January 5 to February 1, February 2 to March 1, March 2 to March 29, April 6 to May 3, and May 4 to May 31, which we loosely refer to below as January, February, March, April, and May. The first three constitute pre-reform periods, while the last two constitute post-reform periods. Notice also that all the periods are four weeks long.

We further drop the first Friday, the second Monday, and the last Friday from each period, as they correspond, in the appropriate month, to public holidays in the Czech Republic (Easter, Labor Day, and Victory Day).

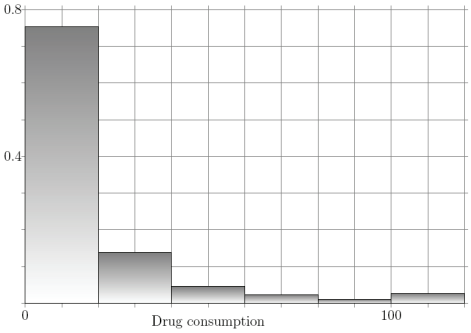
Also, we decided to exclude data from the reform week in order to avoid counting prescriptions issued in the pre-reform period as post-reform ones. We do not observe the date when a given prescription was issued by a doctor, but only the date when it was taken out. As the majority of prescriptions are collected on the issuance day or one day later, omitting the reform week from the data should minimize the risk of this misallocation.

After all the aforementioned adjustments, the resulting dataset contains more than 14,000 prescriptions. *Table 1* shows the number of prescriptions collected by children and adults in the five periods.

Even at this preliminary stage, the number of prescriptions collected by children and adults can be used to calculate the DD estimate of the effect of the reform on the number of children's doctor visits. For example, using January as the pre-reform period and April as the post-reform period (and using the population adjustment explained below) the DD estimate of the policy change effect is an increase in the number of children's prescriptions of 12.65. Using the same post-reform month and February and March as the pre-reform periods gives -16.78 and 36.12, respectively.

The main drawback of this method is that it does not allow us to calculate the statistical significance of the results. And with the three estimates varying in sign (the same applies when May is taken as the post-reform period), it is hard to draw any firm conclusion. For this reason we use data about drug consumption in the remainder of the paper.

Figure 2 Percentage of Drugs with Given Overall Demand



To derive the dataset used for the estimation, we calculated demand for every drug for every period. The resulting dataset contains observations for 1,719 adult drugs and 207 child drugs. For legal reasons, all the data work was done on the computer system of the said pharmacy and the resulting dataset is devoid of any personal details.

As said, the dataset contains information about demand for over 1,900 different drugs, with the demand divided into five different periods. Overall, the demand for individual drugs is not large, as *Figure 2* shows.

The histogram in *Figure 2* shows the percentage of drugs with given overall demand in the observed period, with the last column summarizing all drugs with demand above 100 packs. Most of the drugs (75%) have demand lower than 20 packs and only 2.75% of the drugs have demand higher than 100 packs, with the maximum demand being 530.

Notice that the rather low demand raises no problem for the estimation methodology we use. Nevertheless, to be sure that our results are not being driven by rarely prescribed drugs, another dependent variable we use below drops all drugs with zero consumption in any of the months, effectively constraining the dataset to drugs with consumption of at least five packs in the observed period.

To estimate effect of the reform we use the standard DD strategy introduced originally by Ashenfelter (1978). More formally, we estimate a model of the form

$$dem_{it} = \alpha + \beta T_{it} + \chi post_{it} + \delta ref_{it} + \varepsilon_{it}$$

where i's denote the different drugs, t denotes the pre- and post-reform periods, *dem* denotes consumption of (demand for) a given drug, T is a treatment group dummy, *post* is a post-reform dummy, $ref = T * post$, and ε is a random error. The effect of the reform is then given by the estimate of δ.

For the estimation strategy to be valid, we need two assumptions to be satisfied. First, we need the composition of the control and treatment groups to remain similar over the observed period. This assumption is trivially satisfied, as we are using panel instead of repeated cross-section data.

Second, we need any general trend to affect both the control and treatment groups in a similar way. Indeed, this assumption is often violated in DD studies, an observation made by Ashenfelter (1978), owing either to behavioral changes of eco-

nomic agents before the reform or to selection bias. For this reason we decided to use three pre-reform periods, which allows us to test the assumption of equal trends, if only during the pre-reform period.

Finally, notice that even with a general trend affecting both groups equally, we are running a risk of this general trend confounding our results due to the fact that the adult and child populations are of different size. By way of example, consider a flu epidemic which infects half of the population and each infected person is prescribed a single drug. Then the consumption of this drug increases in proportion to the population, but considerably more for the adult population. This is due to the mechanical effect of the adult population being larger than the child population. For this reason, in all the specifications below we divide demand for adult drugs by 4.6, which is the ratio of adults to children in the Czech population.

The effect of a different probability of a doctor visit generating a prescription for children and adults is similar to the effect of different population sizes. For this reason, our population adjustment further multiplies the adult drug consumption data by 0.77, which is a ratio of 0.74 to 0.96, where the former is probability of a child's doctor visit generating a prescription and the latter is the same probability for adults.[1]

As a final adjustment to the data we multiply adult drug consumption by 0.60, which is a ratio of 1.48 to 2.45, where the former is the average number of packs per child prescription and the latter is the same for adult prescriptions in our dataset. To avoid cumbersome repetition of the full explanation of the adjustments, we simply call it population adjustment.

4. Results

Table 2 shows the results of our estimations using three different pre-reform periods (January, February, and March) and three different dependent variables. The first dependent variable is simply consumption of the given drug. For the second dependent variable we drop all the drugs with zero consumption in any of the pre- or post--reform months. Finally, the third dependent variable uses the more demanding criterion discussed above in order for a drug to be labeled as either a child or adult one, dropping the rest of the data.

What the estimates of *ref* clearly show for all three dependent variables is no reform effect when January or February is taken as the pre-reform period. On the other hand, when March is taken as the pre-reform period the estimates are positive and statistically significant for two out of the three dependent variables.

We read this as an example of the "Ashenfelter dip" identified by Ashenfelter (1978). His study tries to estimate the effect of government training programs on a participant's subsequent income by comparing it to pre-enrolment income. What Ashenfelter noticed is that there is a significant dip in income immediately prior to enrolment. In other words, those who lost their job are more likely to self-select into training programs. As a result of the self-selection, DD estimation using pre-enrolment income from a period close to the enrolment date will overestimate the effect of the training programs.

[1] The data are based on 2008 statistics compiled by the Czech Ministry of Health using information from all insurance companies operating in the Czech Republic. The data have not been officially published and were provided by the Ministry upon request.

Table 2 Difference-in-Difference Estimates of Reform Effect on Children Drug Consumption

dependent variable	(1)	(1)	(1)	(2)	(2)	(2)	(3)	(3)	(3)
base period	January	February	March	January	February	March	January	February	March
constant	0.36	0.32	0.36	1.00	0.86	1.00	0.34	0.31	0.35
	(0.02)***	(0.01)***	(0.02)***	(0.05)***	(0.05)***	(0.05)***	(0.02)***	(0.02)***	(0.02)***
T	1.58	1.67	1.20	5.52	4.70	3.70	1.60	1.99	1.16
	(0.24)***	(0.20)***	(0.17)***	(0.73)***	(0.63)***	(0.51)***	(0.42)***	(0.41)***	(0.28)***
post	0.03	0.06	0.02	0.09	0.22	0.08	0.02	0.05	0.01
	(0.01)**	(0.01)***	(0.01)**	(0.04)**	(0.03)***	(0.03)**	(0.01)	(0.01)***	(0.01)
ref	0.23	0.14	0.62	0.08	0.90	1.90	0.47	0.08	0.91
	(0.27)	(0.27)	(0.27)**	(0.61)	(0.69)	(0.70)***	(0.61)	(0.63)	(0.63)
N	3852	3852	3852	942	942	942	3338	3338	3338
ch/a	1719/207	1719/207	1719/207	430/41	430/41	430/41	1593/76	1593/76	1593/76
R^2	0.21	0.23	0.22	0.60	0.59	0.59	0.18	0.19	0.18

Notes: Dependent variables are demand for given drug (1), demand for given drug with drugs with zero demand dropped (2), demand with more demanding children/adult drug split (3). *T* denotes treatment group dummy, *post* denotes post-reform dummy, *ref* denotes interaction of *T* and *post*. Base period denotes chosen pre-reform period. Post reform period is April. *ch/a* row indicates number of children/adult drugs in regression. Robust clustered (on individual drugs) standard errors in parentheses. ***, **, * significance at 0.01, 0.05, 0.1 level respectively.

Table 3 Test of Equal Trends Hypothesis

dependent variable	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
comparison months	January vs. February			January vs. March			February vs. March		
constant	0.36	1.00	0.34	0.36	1.00	0.34	0.32	0.86	0.31
	(002)***	(0.05)***	(0.02)***	(0.02)***	(0.05)***	(0.02)***	(0.01)***	(0.05)***	(0.02)***
T	1.58	5.52	1.60	1.58	5.52	1.60	1.67	4.70	1.99
	(0.24)***	(0.73)***	(0.42)***	(0.24)***	(0.73)***	(0.42)***	(0.20)***	(0.63)***	(0.41)***
*post*1	-0.03	-0.14	-0.03						
	(0,01)***	(0.03)***	(0.01)***						
*ref*1	0.09	-0.82	0.39						
	(0.19)	(0.62)	(0.28)						
*post*2				0.00	0.01	0.01	0.03	0.14	0.04
				(0.01)	(0.03)	(0.01)	(0.01)***	(0.03)***	(0.01)***
*ref*2				-0.39	-1.81	-0.44	-0.47	-1.00	-0.83
				(0.18)**	(0.59)***	(0.33)	(0.15)***	(0.53)	(0.26)***
N	3852	942	3338	3852	942	3338	3852	942	3338
ch/a	1719/207	1719/207	1719/207	430/41	430/41	430/41	1593/76	1593/76	1593/76
R^2	0.26	0.61	0.24	0.24	0.60	0.22	0.28	0.59	0.25

Notes: DD estimates with fictitious reform set to February (first three columns) or March (last six columns). Dependent variables are demand for given drug (1), demand for given drug with drugs with zero demand dropped (2), demand with more demanding children/adult drug split (3). *T* denotes treatment group dummy, *post*1 denotes post-reform (February) dummy, *post*2 denotes post-reform (March) dummy, *ref* denotes interaction of *t* and *post*. Robust clustered (on individual drugs) standard errors in parentheses. ***, **, * significance at 0.01, 0.05, 0.1 level respectively.

In our case, we read the estimates as showing a strategic timing effect. By mid-February it became clear that starting in April children would be exempt from doctor-visit co-payments. Consequently, some of the doctor visits were postponed, which caused them to drop in March and surge in April. Indeed, the results from the test of the equal trends assumption below confirm this interpretation.

One interesting question is how the estimated strategic changes in consumption of child drugs translate to children's doctor visits. A simple back-of-the--envelope calculation allows us to do this. In the data, the average consumption of child drugs in March and April is 1.56 and 2.19 packets, respectively. For the estimate of the reform effect to be zero those numbers would have to be 1.87 and 1.89, respectively. Furthermore, in our data each child prescription has approximately 1.48 drug packets and we know each child doctor visit generates 0.74 prescriptions.

The visit-to-packet "exchange" rate is thus 0.91 visits per packet. Using the March data, this translates to 1.42 actual visits and 1.70 visits that would have been made without the strategic shift, or a 16% decrease. Using the estimate for our second dependent variable, which drops drugs with zero consumption, would give a 17% decrease. As we explain below, this estimate most probably overestimates the true extent of visit shifting due to other reform changes which took place.

5. Robustness Checks

We now want to discuss the robustness of our findings. First and foremost, we tested the equal trends assumption. To test the assumption for, say, January and February, one constructs a fictitious reform effective in February and then tries to estimate its impact using standard DD estimation. If the trends in consumption of adult and child drugs are the same between the two months, the fictitious reform should have no impact.

Table 3 shows the results. The first three columns show the test of the equal trends hypothesis for January versus February, while the remaining columns test January versus March and February versus March. Focusing on the January-versus-February test, none of the dependent variables gives a significant estimate of the fictitious reform effect. On the other hand, the results for the January-versus-March and February-versus-March tests clearly violate the equal trends assumption. This fits neatly with the strategic shifting explanation discussed above. Given the March decrease in consumption of child drugs, comparing January or February to March should yield a statistically significant and negative estimate of the fictitious reform.

For obvious reasons we are not able to test the equal trends assumption using pre- and post-reform months, when we would indeed like the assumption to be satisfied. This raises the issue of factors that influence the adult and child parts of population in a different way potentially confounding our results.

One such possible factor is allergies, a disease with much higher prevalence among children, combined with spring as the beginning of the pollen season. However, even a casual look at any pollen calendar shows that allergies are not likely to be behind our results, as pollen activity can explain the increase observed between March and April, not the decrease observed between February and March, in the number of children's doctor visits.

Another factor influencing adults and children differently is secondary school admission tests. Those are likely to induce a short-term shift in children's doctor visits in time. Although we are not able to adjust for this effect in the data, we believe it does not lie behind our results, as the magnitude of the shift is likely to be days. Furthermore, in the Czech Republic the period for admission tests is set centrally and in 2009 ran from April 22 to May 7. In other words, it has a considerable overlap with our post-reform period.

Yet another criticism of the results is that if the strategic timing effect away from March toward April is indeed true, then our April data overestimate true child drug consumption. This then implies that the estimates of the reform effect using January and February as the pre-reform periods are likely to overestimate the true reform effect. We regard this as a valid objection, but are unable to filter out this effect. At the same time, given that the estimates using January and February as the pre-reform periods suggest no reform effect, correcting for the overestimate can at best turn the estimates into negative ones, which would be a rather puzzling result.

Next, we conducted a series of further robustness checks. First, our population adjustment operates on the adult data. As an alternative, we experimented with adjustment operating on child drug consumption. Second, we used May as the post-reform period. Third, we tried to account for the fact that our dependent variable takes on integer values only, making use of OLS estimation potentially inappropriate. For this reason, we re-estimated the regressions in *Tables 2* and *3* using count data models. Specifically, we experimented with Poisson and negative binomial models (see Cameron and Trivedi, 2009, chapter 17, for a discussion). In all the cases described, the results are very similar to those already presented and are not reported for the sake of space.

Another issue with our estimates is the possible confounding effect of the third reform change. Recall that this change specifies that prescription co-payments must be paid only in cases where the patient supplementary payment is less than CZK 30. The effect of this change is, conditional on a doctor visit, an increase in the probability of a doctor visit leading to a prescription.

However, this violates our assumption that a doctor visit leads to a prescription with a fixed probability and makes the correspondence between drug consumption and doctor visits much looser. However, note that we can allow the probability not only to vary between children and adults, but also to vary across periods provided the change is the same for children and adults. And as the third reform change increased the probability of a doctor visit generating a prescription for both groups, we think it does not lie behind our results. This also shows the importance of the population adjustment, as the effect of the third change is similar to the effect in the flu example discussed above.

Notice also that any potential bias the third change introduces is likely to lead to an overestimate of the effect of the reform. Given, then, that we find no effect of the reform overall, this is reassuring. This is also why we think the back-of-the-envelope calculation of the extent of the strategic March-to-April shift in doctor visits is an upper bound on any true extent of this shift.

Another possible change induced by the third reform change is substitution towards more expensive drugs. While likely, this does not seem to compromise our

results provided the substitution is towards more expensive drugs of the same (child/ /adult) type. Nevertheless, the possibility of price-induced substitution reveals a drawback of the methodology used, as it does not include prices of drugs in the regression estimation. There are two reasons for this. First, for 70% of the drugs in our data the price is zero, as they are covered fully by the public health system. Second, although the prices change from the pre- to post-reform period, the change does not differ for children and adults.

However, to be sure that the results presented are not driven by the omission of prices, we re-estimated the regressions above with a price variable. The estimates of this richer model, which we do not present for the sake of space, show that the effect of prices is in general not significant and does not change the results presented.

Overall, the third reform change seems not to lie behind the results presented, as it influenced the whole population, something that the DD methodology is designed to deal with. On the other hand, the first reform change influenced only children, making our results vulnerable to scenarios that include some reform effect not observed in our data. One such objection is that with doctor-visit co-payments, patients try to "get the most out of their visit". After the reform, this motivation would be gone for children.

Children not trying to get the most out of their doctor visit can mean, for example, a decrease in the probability of receiving a prescription. Combined with an increase in the number of doctor visits this can leave consumption of child drugs constant, producing the estimates presented, despite the increase in children's doctor visits, i.e., despite the reform having an impact.

Despite the fact that with the current data we are not able to test this interpretation, we think that getting the most out of one's visit would manifest itself also in a decrease in the number of packs prescribed per child prescription after the reform. To test this hypothesis, we calculated the average number of packs per child prescription before and after the reform and tested a null of no difference against an alternative of the average being lower after the reform. The *p*-value of the standard *t*-test is 0.99, strongly rejecting the alternative (the average is in fact slightly larger after the reform).

Yet another potential confounding factor is the one-week spring vacation that children have. This falls into the last week of February for Prague districts 1 through 5 and the first week of March for Prague districts 6 through 10. It is not, however, clear how this influences child drug consumption and doctor visits. On the one hand, a number of children probably leave Prague. On the other hand, those remaining indulge in out-of-school activities with a potentially increased probability of requiring medical attention.

Also, given the rather high mobility of people within Prague it is likely that any considerable vacation effect would show up in both February and March. Thus explaining the March drop in child drug consumption as a vacation effect, there would have to be a comparable effect in February. This would translate to a positive estimate of the reform effect with February as the pre-reform period in *Table 2*. Furthermore, it would translate to a negative estimate of the fictitious reform in the equal trends assumption test comparing January and February in *Table 3*. However, the estimates do not show such an effect.

Table 4 Further Estimation of Shifting Hypothesis

dependent variable	(1)	(2)	(3)	(1)	(2)	(3)
comparison months	one week around reform			three weeks around reform		
constant	0.22	0.40	0.22	0.42	0.68	0.42
	(0.01)***	(0.02)***	(0.01)***	(0.02)***	(0.03)***	(0.02)***
T	1.07	2.02	1.15	1.66	2.85	1.82
	(0.15)***	(0.28)***	(0.30)***	(0.21)***	(0.34)***	(0.36)***
post	0.03	0.05	0.02	0.03	0.05	0.01
	(0.01)**	(0.02)**	(0.01)	(0.01)**	(0.02)**	(0.01)
ref	0.87	0.61	1.67	0.76	0.80	1.22
	(0.51)*	(0.39)	(1.31)	(0.38)**	(0.43)*	(0.98)
N	1756	614	1474	2736	1394	2324
ch/a	790/88	278/29	705/32	1229/139	631/66	1115/47
R^2	0.23	0.68	0.19	0.31	0.53	0.27

dependent variable	(1)	(2)	(3)
comparison months	April vs. May		
constant	0.38	1.08	0.36
	(0.02)***	(0.06)***	(0.02)***
T	1.81	5.60	2.07
	(0.30)***	(0.77)***	(0.67)***
post	-0.02	-0.05	-0.01
	(0.01)	(0.03	(0.01)
ref	-0.29	-0.90	-0.93
	(0.27)	(0.59)	(0.62)
N	3852	942	3338
ch/a	1719/207	430/31	1593/76
R^2	0.21	0.55	0.17

Notes: See *Table 3*. First (second) three columns: data constrained to a single week (three weeks) around reform. Last three columns: estimation with fictitious reform in May.

To shed further light on this issue, we restricted our March data to the last three weeks and the April data to the first three weeks in order to avoid the confounding factor of the spring vacation. The results of the estimation are given *Table 4* along with a further restriction of our data to a single week around the reform. Furthermore, the last three column of the table show the results of the estimation with a fictitious reform in May. If our shifting hypothesis is indeed correct, we should observe a statistically significant drop in child drug consumption between April and May.

The results in *Table 4* send a mixed message. In the first six columns, which use restriction of the data into single or three-week periods around the reform, only three out of the six estimates are statistically significant despite the rest coming close to statistical significance (*p*-values of 0.12, 0.20, and 0.21, respectively).

The estimates for the fictitious reform in May given in the last three columns also have the correct expected sign, but come only close to being significant (*p*-values of 0.29, 0.13, and 0.13, respectively). Overall, we find it hard to discard the shifting hypothesis altogether, despite the fact that on purely statistical grounds it should probably be rejected.

6. Conclusion

We have tried to estimate the effect of the recent change in the structure of public health sector co-payments on the number of doctor visits by children. The main goal of the co-payments has been to limit wasteful use of health sector services. However, we believe that for certain groups of the population this wasteful use is rather limited and any decrease in health care utilization would come at the expense of health outcomes. That is why we focus on children as a prime example of such a group.

We are able to use an April 2009 policy change that waived children's doctor--visit co-payments as a natural experiment. As the adult part of the population did not experience this policy change, we use adults as a control group in the difference-in--difference estimation framework.

Using child drug consumption as a proxy for the number of children's doctor visits, we find no effect of the reform. On the other hand, the data reveal a short-term strategic timing effect associated with the policy change. With the policy effective since April 2009, there is evidence of a shift in children's doctor visits away from March towards April. However, we are not able to fully disentangle the potential confounding effect of the spring vacation and as a result are much less convinced of this result.

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